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A MODEL FOR DEONTIC LOGIC

By E. E. DAWSON

1. *Introduction.* Deontic logic (i.e. the logic of obligation, permissibility, etc.) has, in the last few years, undergone an extensive development. A preliminary systematization by von Wright[1,2] has been given a more powerful formulation by Anderson,[3] and important clarification of Anderson's position has been provided by Prior.[4] Anderson's reformulation involves, essentially, defining the specifically deontic operators O, P, and F (read ' obligatory ', ' permitted ', and ' forbidden ') in terms of the usual alethic operators, L and M (' necessary ' and ' possible'), and a propositional constant S, interpreted as a sanction, and within the framework of a standard modal system (e.g. von Wright's M) supplemented by some appropriate deontic axiom (e.g. KMSMNS). This systematization has the double advantage of bringing more developed logical procedures to bear upon deontic logic and of facilitating the investigation of combined deontic-alethic formulae.

The purpose of the present paper is likewise to bring more powerful methods to bear upon deontic logic by showing that a model satisfying Anderson's requirements for a ' normal deontic logic '[5] is provided by a specific alethic modal system, unsupplemented by deontic axioms, in which ' O ' is represented by ' ML ', and ' P ' by ' LM '. It will also be shown that, with this interpretation, the model satisfies several theorems which von Wright, Anderson, and Prior provide as characteristic of deontic logic, and, further, the model rejects unacceptable formulae.

[1] G. H. von Wright, ' Deontic Logic ', *Mind*, vol. 60 (1951), pp. 1–15.
[2] *An Essay in Modal Logic*, Amsterdam, 1951.
[3] A. R. Anderson, *The Formal Analysis of Normative Systems*, Technical Report No. 2, U.S. Office of Naval Research Contract SAR/Nonr-609(16), 1956.
[4] A. N. Prior, *Time and Modality*, Oxford, 1957, Appendix D, pp. 140–145.
[5] Anderson, *op. cit.*, p. 29.

2. *Development of the Model.* Anderson's conditions for a ' normal deontic logic ', D, are that D shall contain the theorems and principles of the propositional calculus, including the principle of intersubstitutability of materially equivalent expressions, and, in addition, shall contain as theorems COpPp, EPApqAPpPq, and EOpNPNp, but neither CPpp nor CpPp shall be a theorem of D; and, further, if D is an extension of an alethic modal logic, then CMpPp shall not be a theorem. In the proposed model, with O=ML and P=LM, these formulae are translated as follows (where ' ⊢ ' means ' asserted ' or ' it is a theorem that ', and ' * ' means ' rejected ' or ' it is not a theorem that '):

⊢ COpPp	=	⊢ CMLpLMp
⊢ EPApqAPpPq	=	⊢ ELMApqALMpLMq
⊢ EOpNPNp	=	⊢ EMLpNLMNp
* CPpp	=	* CLMpp
* CpPp	=	* CpLMp
* CMpPp	=	* CMpLMp

Prior[1] has shown that the expression MNS, which is Anderson's added axiom to formulate a system of deontic logic as an extension of alethic modal logic[2], is deductively equivalent in most modal systems to COpPp. This latter formula is equivalent in our translation to CMLpLMp, and the model of deontic logic which the present paper is to investigate is the alethic modal system formed by adding, as the only additional axiom, the formula CMLpLMp to the standard Lewis modal system S4.[3] We shall refer to this system as S4.2,[4] and we may show that all of the above asserted formulae are theorems of S4.2 and also that the rejected formulae are demonstrably not in S4.2.

The formula CMLpLMp is a theorem of S4.2, since it is an axiom of the system, while EMLpNLMNp also holds in S4.2—and indeed in any system in which L=NMN and M=NLN hold. The expression ELMApqALMpLMq is equivalent to the conjunction of CALMpLMqLMApq and CLMApqALMpLMq; the proof of the first is as follows:

[1] Prior, *op. cit.*, p. 141.

[2] Anderson, *op. cit.*, p. 34.

[3] Throughout the present paper, the basis for the system S4 will be that provided by Gödel (K. Gödel, ' Eine Interpretation des intuitionistischen Aussagenkalküls ', *Ergebnisse eines mathematischen Kolloquiums*, Heft 4 (1933), pp. 39–40); since this formulation contains the rule that if α is a thesis, so is Lα, it will not be necessary to express any formulae with a preliminary ' L '.

[4] The designation ' S4.2 ' for S4+CMLpLMp is due to Mr. E. J. Lemmon; this system has been quite extensively investigated by, among others, Lemmon, Prior, Mr. P. T. Geach and myself.

1 CLKpqKLpLq	
2 CMKpqKMpMq	
3 CMLKpqMKLpLq	(1 ×α →Lα, and using CLCpqCMpMq)
4 CMLKpqKMLpMLq	(C2, with p/Lp, q/Lq, —C3—4)
5 CNKMLpMLqNMLKpq	(ECpqCNqNp)
6 CANMLpNMLqNMLNANpNq	(ENKpqANpNq and EKpqNANpNq)
7 CANMLNpNMLNqNMLNApq	(6, with p/Np, q/Nq, and using ENNpp, ENNqq)
8 CALMpLMqLMApq	(by modal equivalents)

while the proof of the converse is accomplished using CMLpLMp:

1 CLpCLqLKpq	
2 CMLpMCLqLKpq	(1 ×α →Lα, and using CLCpqCMpMq)
3 CMLpMANLqLKpq	(ECpqANpq)
4 CMLpAMNLqMLKpq	(EMApqAMpMq)
5 CMLpANLqMLKpq	(modal equivalents, and ELLpLp)
6 CMLpCLqMLKpq	(EANpqCpq)
7 CLqCMLpMLKpq	(ECpCqrCqCpr)
8 CMLqMCMLpMLKpq	(7 ×α →Lα, and using CLCpqCMpMq)
9 CMLqMANMLpMLKpq	(ECpqANpq)
10 CMLqAMNMLpMLKpq	(EMApqAMpMq, and EMMpMp)
11 CMLqCLMLpMLKpq	(modal equivalents, and EANpqCpq)
12 CLMLpCMLqMLKpq	(ECqCprCpCqr)
13 CMLpLMLp	(CMLpLMp, with p/Lp, ELLpLp)
14 CMLpCMLqMLKpq	(C12—C13—14)
15 CKMLpMLqMLKpq	(ECpCqrCKpqr)
16 CNMLKpqNKMLpMLq	(ECpqCNqNp)
17 CNMLNANpNqANMLpNMLq	(EKpqNANpNq, and ENKpqANpNq)
18 CNMLNApqANMLNpNMLNq	(17, with p/Np, q/Nq, and using ENNpp, ENNqq)
19 CLMApqALMpLMq	(by modal equivalents)

We may now turn our attention to the three formulae to be rejected. That CLMpp is not in S4.2 is shown by matrix Group II, given by Lewis and Langford,[1] which satisfies S4.2 but not CLMpp; and the same matrix also fails to satisfy CpLMp and CMpLMp, thus showing that they are not in S4.2 either. We may just note also that if CLMpp *were* in S4.2, the system would collapse into material implication, while if either CpLMp or CMpLMp were in S4.2, it would be equivalent to S5.[2] And neither the propositional calculus nor S5 satisfy Anderson's conditions for a normal deontic logic, under the present interpretation.

3. *Further Application of the Model.* A feature wherein deontic modal logic differs from ordinary alethic logics is that while CLpp is a thesis of the latter, the corresponding formula COpp is not a theorem of the former. However, the stronger expression OCOpp *is* commonly taken to be a law of deontic logic.[3] Our model reflects these features: CMLpp is *not* in S4.2 (as is shown by the matrix Group II, referred to above), but the expression MLCMLpp *is* in S4.2 (and indeed is a theorem of S4 without the added axiom); the proof[4] is as follows:

1	CLMMLpMLp	
2	CMKpqKMpMq	
3	LCMKpqKMpMq	$(2 \times \alpha \rightarrow L\alpha)$
4	CLMKpqLKMpMq	(CLCpqCLpLq)
5	CLMKpqKLMpLMq	(ELKpqKLpLq)
6	CLMNCpNqNCLMpNLMq	(EKpqNCpNq)
7	CLMNCpqNCLMpNLMNq	(6, with q/Nq, and ENNqq)
8	CCLMpMLqMLCpq	(ECpNqCqNp, and modal equivalents).
9	CCLMMLpMLpMLCMLpp	(8, with p/MLp, q/p)
10	MLCMLpp	(C1—C9—10)

Likewise, while CpPp, the deontic analogue of the alethic thesis CpMp, is unprovable, the stronger form OCpPp *is* provable, under our interpretation.

In von Wright's system of deontic logic, acts (or act-types)

[1] C. I. Lewis and C. H. Langford, *Symbolic Logic*, New York, 1932, Appendix II, p. 493.

[2] These points are based on proofs due to Parry (W. T. Parry, 'Modalities in the Survey System of Strict Implication', *Journal of Symbolic Logic*, vol. 4 (1939), pp. 137–154.)

[3] See Prior, *op. cit.*, p. 140; also A. N. Prior, *Formal Logic*, Oxford, 1955, p. 225.

[4] I am here indebted to Mr. P. T. Geach for suggesting corrections in an earlier proof.

rather than propositions are taken as arguments for the deontic operators P and O, but if we substitute propositional variables for act variables throughout, the resulting system is completely satisfied in this model. Thus von Wright's three principles become (in symbolic form) PApq←→APpPq, Pp v PNp, and LEpq →LEPpPq; and, of these the first is provable directly from ELMApqALMpLMq, the second is provable from CMLpLMp, and the third is proved from CLCpqCMpMq, using α←→Lα, CLCpqCLpLq, CLpLLp, CCpqCCrsCKprKqs, and the definition LEpq=KLCpqLCqp. Of the P-tautologies which are instanced—1) CKOpOCpqOq, 2) CKPpOCpqPq, 3) CKNPqOCpqNPp, 4) CKKOCpAqrNPqNPrNPp, 5) NKKOApqNPpNPq, 6) CKOpOCKpqrOCqr, and 7) COCNppOp—it may readily be shown in the model that all are satisfied and that 1), 2) and 3) are equivalent and imply 4), 5) and 6).

The present alethic interpretation of the deontic operators also provides a straightforward means of resolving the status of combined deontic-alethic formulae. For instance, the 'Kantian principle' COpMp is trivially a theorem, and so also is CPpMp. And likewise with CLpOp, CLpPp. and CNMpONp[1]. A slightly more complex point is the following: it may be shown that CKPpPqPKpq is not a theorem (by matrix Group III of Lewis and Langford), and it is evidently not very plausible[2]; but the expression CKPpPqMPKpq has, perhaps, a greater intuitive appeal. However, it too is rejected in our model. The general form CKαpαqMPKpq seems to me to be of some interest: what must be the deontic status, α, of two activities for it to be possible that it is permitted to do both of them? A sufficient condition, certainly, is that they both be obligatory; CKOpOq-MPKpq; but what if they are only *possibly* obligatory: CKMOp-MOqMPKpq? or *necessarily* permitted: CKLPpLPqMPKpq? In the present model, 'possibly obligatory' and 'necessarily permitted' collapse into the simple 'obligatory' and 'permitted', respectively, so that the first formula holds and the second does not. It may be, indeed, that the expressions 'possibly obligatory' and 'necessarily permitted' are without deontic sense,[3] but this is a question of ordinary linguistic usage on which our model casts no light.

[1] These formulae are in Anderson, *op. cit.*, p. 53.
[2] See *ibid.*, p. 50.
[3] However, both von Wright, *Essay*, p. 41, and Anderson, *op. cit.*, p. 74 use these expressions.

4. *Remarks.* It has been the purpose of this paper to investigate the structure of deontic logic, not as a separate branch of logic (as does von Wright) and also not as a deontic-supplemented alethic logic (as does Anderson), but as an ordinary alethic modal logic. The model here developed, then, is a ' picture of reality ' (reality in this case being ordinary language) in which ' ML ' stands for the phrase ' it is obligatory that ' and ' LM ' stands for ' it is permitted that ', and the adequacy of the model is determined by how well it ' fits ' the reality. It may well be that it is too strong—that it admits formulae as theorems which under the above interpretation yield expressions which are not plausible—although I have not been able to find any such. Again, it may well be too weak, in that there are non-theorems whose interpretation we *do* find plausible; in this connexion, I believe there may be something to be said for CLPpMOp, which, as CLLMpMMLp, is not a theorem, but does appear to be interesting linguistically. Note that the formula cannot be added to the present model without vitiating it.

From a formal point of view, S4.2 is the weakest alethic model which, with the suggested interpretation, will satisfy Anderson's requirements for a normal deontic logic, since we required both CMLpLMp and ELLpLp in proving the deontic theorems (although there are other ways of forming the system S4.2 than by adding CMLpLMp to S4). An interesting candidate for a stronger model is the system formed by adding CKpMLpLp to S4; the interpretation of this—' if what is done is obligatory, then it *must* be done (it is *not possible* not to do it) '—has a rather hard and deterministic ring; we shall not study this system in the present paper.[1]

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[1] Mr. P. Geach has pointed out to me that not only is the system S4 + CKpMLpLp a ' hard and deterministic ' model for deontic logic, but it is positively immoral as well. For one may prove not only ' what is obligatory is done only if it is unavoidable ' but also ' anything that is forbidden is done if it is possible ' (CKMpMLN pp), which may have a certain appeal, but is hardly acceptable as a basis for an *ethical* deontic logic! It is an axiom only appropriate for a world of ' totally abandoned criminality '.

TOWARDS A THEORY OF DEFINITE DESCRIPTIONS

By K. J. J. HINTIKKA

THE inference

(A) Schopenhauer is a philosopher who had a high opinion of Schopenhauer's work; therefore there is at least one philosopher of whose work Schopenhauer had a high opinion

seems impeccable. (A more idiomatic form is obtained by replacing " Schopenhauer's " by " his own ".) It is therefore not surprising to see that the general principle which underlies (A) is incorporated in those current formulations of quantification theory (predicate calculus) which employ free individual variables. In them, one may infer an existentially quantified formula from any formula obtainable from it by omitting the quantifier and by replacing the variable which was bound to the quantifier by a free one. If we use 'f', 'g', . . . to refer to arbitrary formulae,[1] 'x', 'y', . . . to refer to arbitrary bound individual variables, 'a', 'b', . . . to refer to arbitrary free individual variables, and '$f(a/x)$' to refer to the result of replacing x by a in f, we may express the principle in question concisely by

(B) $$f(a/x) \rightarrow \ulcorner(\exists x)f\urcorner.$$

(B) is known as the rule of *existential generalization*. The ' corners ' appearing in (B) are Quine's quasi-quotes;[2] they will be used consistently in the sequel. The arrow does not refer to any feature of a formal system. Its role in the discussion of the interrelations of formulae will be explained later. Meanwhile, we may provisionally think of it as standing for such English words as ' therefore ' or ' hence '.

However, (B) soon gives rise to trouble. The following piece of reasoning seems as good an instance of (B) as (A):

(C) Homer was a Greek poet who composed the *Iliad;* therefore there was a Greek poet who composed the *Iliad.*

But if the premise and the conclusion of (C) are taken in one very natural sense, (C) is a *non sequitur*. In fact, we find a historian

[1] Strictly speaking, to formulae or to expressions which are like formulae except for containing bound variables in the place of free ones. I shall restrict the term ' formula ' to expressions in which every bound variable is actually bound to some quantifier.

[2] W. V. Quine, *Mathematical Logic* (rev. ed., Cambridge, Mass., 1951), § 6.

stating that " the question, 'Was there a Homer?' is a very pertinent one " soon after having pointed out that " to the query 'Who was Homer?' one can hardly answer anything except 'the author of the *Iliad*' ".[1] Since our historian presumably takes Homer's nationality and profession for granted, he is bound to reject (C).

This does not so much prove that there is something wrong with the usual formulations of quantification theory as show that some explanation is needed concerning their applicability to ordinary discourse even in contexts in which the structure of ordinary language is clear enough to invite such an application. It may be taken for granted that genuine names can always be substituted for the free individual variables of a formally correct inference. For what are free individual variables but placeholders for proper names? Any attempt to deny that (C) is obtained from (B) in an appropriate way may therefore be dismissed. However, there remain at least three ways of explaining away the inadmissible applications. One may (i) try to explain away the names that give rise to trouble, (ii) dispense with free variables, or (iii) modify the rules of formal logic so as to get rid of (B).

Russell's well-known treatment of proper names may be taken to be an instance of (i). It illustrates the difficulties into which (i) inevitably leads irrespective of the peculiarities of Russell's logical atomism. The reason why (C) is objectionable is that we do not know for sure whether the name 'Homer' had a bearer or not, for existential generalization with respect to a term is a valid inference only if the term in question really refers to something or somebody. But from the point of view of logic there is little to choose between different names. The question whether a putative name has a bearer is always a contingent one; such questions as 'Did N.N. really exist?' cannot be answered by purely logical means. Hence it does not suffice to discard names which are admittedly empty; one is involved in the quixotic attempt to explain away all or nearly all proper names.

The approach (ii), which is somewhat in Quine's spirit, is more promising. We know that quantification theory can be developed without using free individual variables at all. Hence there is no need to admit such sources of trouble as (B), although we must have an analogue to (B) for bound individual variables.

However, this approach poses the problem of explaining how a quantification theory in which all individual variables are

[1] George Sarton, *A History of Science* (Cambridge, Mass., 1952), pp. 130, 133.

bound can be applied to the singular terms of ordinary discourse, for in the absence of free individual variables there is in the formal system nothing for which proper names can be substituted. Although such an explanation seems to be forthcoming, it does not preclude an attempt to make the adjustment in a more immediate way by resorting to (iii). This means that we have to reject (B) as a rule of formal logic. In general, we have to reject all the inferences that fail if empty proper names are substituted for their free individual variables.

In a paper forthcoming in the *Journal of Philosophy*, I have outlined a way of constructing a ' quantification theory without existential presuppositions ' to this effect. The resulting system is in terms of a dyadic metalogical relation which is called the relation of equivalence and expressed by ' $\longleftrightarrow$ '. This relation is assumed to be transitive, and occurrences of equivalent expressions are assumed to be interchangeable irrespective of the context. The relationship of which (B) is an instance may be defined in terms of ' $\longleftrightarrow$ ': ' $f \rightarrow g$ ' is just another way of expressing that $f \longleftrightarrow \ulcorner(f \,\&\, g)\urcorner$.

Taking propositional logic for granted, we may formulate the basic rules of the new system as follows:

(1) Two formulae which are tautologously equivalent by the propositional calculus are equivalent provided that they contain exactly the same free variables, and so are expressions obtained from such formulae by replacing one or more free individual variables by bound ones.

(2) $f(y/x) \rightarrow \ulcorner(\exists x)f\urcorner$.

(3) If g does not contain x, then $\ulcorner(\exists x)\,(f \,\&\, g)\urcorner \longleftrightarrow \ulcorner((\exists x)f \,\&\, g)\urcorner$.

(4) (a) If f contains an occurrence of x which is not bound to any quantifier (or to any description-operator), $f \rightarrow \ulcorner x = x\urcorner$.

(4) (b) If a occurs in f, $f \rightarrow \ulcorner a = a\urcorner$.

(5) (a) $\ulcorner x = y \,\&\, f(y/x)\urcorner \rightarrow f$.

(5) (b) $\ulcorner x = a \,\&\, f(a/x)\urcorner \rightarrow f$.

(5) (c) $\ulcorner a = b \,\&\, f(b/a)\urcorner \rightarrow f$.

If (B) is adjoined to this set of rules, we obtain a system which is equivalent to the usual quantification theory in the sense that $f \longleftrightarrow \ulcorner(f \vee \sim f)\urcorner$ holds if and only if f is valid in every

domain of individuals (including the empty one). Hence the new system which dispenses with existential presuppositions may be said to result from the old one simply by renouncing the rule of existential generalization as applied to free variables.

In the new system, we can reconstruct inferences that depend on the success of names in naming by introducing suitable contingent premises which insure that the terms in question are not empty. For instance, (A) is conceived of as an enthymeme which depends on the tacit additional premise ' There is such a person as Schopenhauer ' (with a non-temporal ' is ', of course). It turns out that such formulae as $\ulcorner(\exists x)(x = a)\urcorner$ may serve as the formal counterparts of these premises. For if this formula is added as a premise, we are entitled to use the rule of existential generalization. To put the same point in formal terms: it can be shown that

(6) $\quad \ulcorner(\exists x)(x=a)\ \&\ f(a/x)\urcorner \rightarrow \ulcorner(\exists x) f\urcorner.$

(For a proof, see my article in the *Journal of Philosophy*.) Since the rule of existential generalization is the only thing that distinguishes empty names from names with a bearer, (6) implies that $\ulcorner(\exists x)(x = a)\urcorner$ makes a behave in all respects like a name with a bearer.

This strongly supports Quine's thesis that " to be is to be a value of a bound variable ", for what $\ulcorner(\exists x)(x = a)\urcorner$ says is just that the individual referred to by a is identical with a value of the bound variable x. However, I shall not deal directly with Quine's thesis in this paper. Instead, I shall sketch a theory of definite descriptions which differs from Russell's and which avoids, it seems to me, the mistakes of Russell's. The existence of such a theory of descriptions is not without relevance to Quine's thesis, for it shows that the thesis may hold, under one interpretation at least, while Russell's theory does not. In short, it shows that Quine's thesis is independent of Russell's theory.

Formally, descriptions may be accommodated by admitting them to our formulae on the same footing with free individual variables.[1] From every formula f which contains a but not x we obtain the description $\ulcorner(\imath x) f(x/a)\urcorner$ which may be read $\ulcorner$the x such that $f(x/a)\urcorner$.

[1] Unlike some other non-Russellian treatments of definite descriptions (*e.g.* the one by D. Hilbert and P. Bernays in *Grundlagen der Mathematik*, vol. 1, Berlin, 1934) our theory thus admits descriptions into formulae even when the existence and uniqueness of the described object has not been proved. Since free individual variables are doing duty for proper names, the admission of descriptions on the same footing with free individual variables in effect assimilates descriptions to (ordinary) proper names.

What laws govern the use of such descriptions? This question may be approached by asking, Why did Russell's theory go wrong? In trying to diagnose the failure of Russell's theory, some philosophers of ordinary language have come out with a long list of logical maladies. I suspect that the case is much simpler. When we use a definite description, we are primarily interested in the case in which it fits a unique person or object. If it does not, we do not have much use for the description, and do not care how it fares. For this reason, any attempt to impose a uniform interpretation on statements containing empty descriptions is likely to yield unnatural results.

If this diagnosis is correct in the main, there is little to be said about the properties of a described object unless we know that it exists. Formally speaking, we cannot eliminate a definite description from a given formula unless there is an extra premise which insures that the description is not empty. But in any case we have to be able to say whether a given object is or is not the object described. If there is anything that we do know about definite descriptions, that is what it means for something or somebody to be the so-and-so: it means to be a so-and-so, and a unique one. Mr. Nixon is the vice-president of the United States, since he is a vice-president of the U.S. and since there are no others. Mr. Kennedy is not the Senator from Massachusetts, since the Bay State is also represented in the U.S. Senate by Mr. Saltonstall.

This means that we can analyze identities with a definite description on one side. We have

(7) (a) $\ulcorner a = (\iota x) f \urcorner \longleftrightarrow \ulcorner f(a/x) \;\&\; (x)(f \supset x = a) \urcorner$ and

(7) (b) $\ulcorner y = (\iota x) f \urcorner \longleftrightarrow \ulcorner f(y/x) \;\&\; (x)(f \supset x = y) \urcorner$.

(7) (a)–(b) may also be shown to result from Russell's theory. Hence this is a case in which Russell's theory gives correct results. And it is interesting to see that its other correct results are largely consequences of (7).[1]

For instance, (7) (b) enables us to explicate what it means for the so-and-so to exist. It was pointed out above that the formula $\ulcorner (\exists x)(x = a) \urcorner$ serves to guarantee that a is not empty. Consequently, what we have to do in order to explain what it means for $\ulcorner (\iota x) f \urcorner$ not to be empty, is to set up an equivalence with $\ulcorner (\exists y)(y = (\iota x) f) \urcorner$ on one side:

[1] The fact that the results of Russell's theory are in some cases much more natural than in others was pointed out by P. Geach in ' Russell's Theory of Descriptions ', ANALYSIS, vol. 10 (1950) (reprinted in *Philosophy and Analysis*, ed. by Margaret Macdonald, Oxford, 1954).

(8) $\ulcorner(\exists y)\,(y=(\imath x)f)\urcorner \longleftrightarrow \ulcorner(\exists y)\,(f(y/x)\ \&\ (x)(f\supset x=y))\urcorner$

This follows immediately from (7) (b) by the substitutivity of equivalents. (8) is intuitively acceptable: there is such a person as the King of England if and only if there is a unique King of England. As a consequence, we can also explain what it means for the so-and-so not to exist.

Moreover, it can be explained what it means for the so-and-so to have further properties, provided that it really exists:

(9) $\ulcorner(\exists y)(y=(\imath x)f)\ \&\ g((\imath x)f/y)\urcorner \longleftrightarrow$
$\ulcorner(\exists y)\,(f(y/x)\ \&\ g\ \&\ (x)(f\supset x=y))\urcorner$

where x is assumed not to occur in g. This may be proved by means of (6) and (7) (b). For the purpose, I shall first prove the following simple lemma which inverts (5) (b):

(10) $\ulcorner x=a\ \&\ f\urcorner \rightarrow \ulcorner f(a/x)\urcorner.$

Proof: $\ulcorner x=a\ \&\ f\urcorner$
(by (1)) $\longleftrightarrow \ulcorner(x=a\ \&\ f\ \&\ f(a/x))\vee(x=a\ \&\sim f(a/x)\ \&\ f)\urcorner$
(by (5)(b)) $\longleftrightarrow \ulcorner(x=a\&f\&f(a/x))\vee(x=a\ \&\sim f(a/x)\ \&\sim f\&f)\urcorner$
(by (1)) $\longleftrightarrow \ulcorner(x=a\ \&\ f\ \&\ f(a/x))\urcorner$

By means of (10) we can now prove (9):

$\ulcorner(\exists y)(y=(\imath x)f)\ \&\ g((\imath x)f/y)\urcorner$
(by (3)) $\longleftrightarrow \ulcorner(\exists y)\,(y=(\imath x)f\ \&\ g((\imath x)f/y))\urcorner$
(by (5)(b)) $\longleftrightarrow \ulcorner(\exists y)(y=(\imath x)f\ \&\ g((\imath x)f/y)\ \&\ g)\urcorner$
(by (1) & (10)) $\longleftrightarrow \ulcorner(\exists y)(y=(\imath x)f\ \&\ g)\urcorner$
(by (7)(b)) & (1)) $\longleftrightarrow \ulcorner(\exists y)(f(y/x)\ \&\ g\ \&\ (x)\,(f\supset x=y))\urcorner.$

Thus we can see that a theory based on (7) gives us the same results as Russell's theory if the existential presupposition is fulfilled. However, our theory is in general weaker than Russell's. In particular, no transformation like (9) can be carried out if the premise $\ulcorner(\exists y)(y=(\imath x)f)\urcorner$ is not present. The unnatural consequences of Russell's theory are therefore absent.

The fact that our theory is actually weaker than Russell's may be demonstrated very easily provided that we do not admit descriptions within descriptions.[1] Consider formulae which contain only one free individual variable a and no predicate-variables. (The second condition means that the only context in which individual variables may appear outside quantifiers and description-operators is in identities). It is readily seen that all my rules preserve this characteristic, provided of course that in (7) f is stipulated not to contain any descriptions. For

[1] This assumption is not necessary, but it greatly simplifies the matter.

certain formulae with this characteristic, we may define a truth-value by means of the following conventions: $\ulcorner a = a \urcorner$ is true; $\ulcorner a = (\iota x) f \urcorner$ or $\ulcorner (\iota x) f = a \urcorner$ is true if and only if $\ulcorner f(a/x) \urcorner$ is true; $\ulcorner (\iota x) f = (\iota y) g \urcorner$ is true if and only if $\ulcorner f(a/x) \urcorner$ and $\ulcorner g(a/y) \urcorner$ have the same truth-value. Propositional connectives are assumed to obey their usual truth-value rules; every formula beginning with $\ulcorner (\exists x) \urcorner$ is false and every formula beginning with $\ulcorner (x) \urcorner$ is true. It is seen that this defines a unique truth-value for every formula of the sort I described provided that all its bound variables are actually bound to some quantifier or to some description-operator $\ulcorner (\iota x) \urcorner$. The reader may verify that this truth-value is preserved by all the rules (1)–(7). In contrast, the following transformation fails to preserve truth-value:

(D) $\ulcorner a = (\iota x)(a = x) \urcorner \longleftrightarrow \ulcorner (\exists y)(a = y \,\&\, (x)(a = x \supset x = y)) \urcorner$.

(D) is authorized by Russell's theory; and it also results from (9) if the existential premise is discarded (by taking f to be $\ulcorner a = x \urcorner$ and g to be $\ulcorner a = y \urcorner$). It follows that (D) is not a consequence of my theory although it is acceptable to Russell. Also, it follows that the existential premise cannot be omitted from (9).

It seems to me that the major interest of the new theory of descriptions lies in the methods used in it rather than in their results. In other words, it mainly serves to illustrate the virtues of our new quantification theory which dispenses with existential presuppositions. I do not think that the new theory of descriptions holds in all circumstances. For instance, (7) (a) probably fails in contexts in which we are not interested in the actually existing objects but rather in fictions of some sort. Nevertheless the new theory is obviously a much more faithful representation of the logic of ordinary discourse than Russell's, and I do not see why the possible aberrations from it could not also be dealt with by methods not unlike those used here.

Harvard University

IMAGINING THE IMPOSSIBLE

By NORWOOD RUSSELL HANSON

I

IF I can imagine X, then X is not logically impossible. If X can be thought, then " X " is consistent. If one could draw a picture of X, then " X " is not self-contradictory.

What justifies these claims? A statement as to what is or is not possible, is a logical statement. It tells us whether or not some purported description is of the form P.~P. The claim that X is logically possible just is the claim that X's description is not of the form P.~P. And such a claim, if true, could not but be true. The claim that a quadrilateral triangle is impossible, is the claim that " X is a quadrilateral triangle " is a description which can be reduced to the form P.~P. If this claim is true, it necessarily could not be false.

Prima facie, however, it does not seem that a statement about what one can and cannot imagine, or think, or picture, concerns logic. It is in some sense contingent to say of me that I can imagine that X, while you cannot imagine that X. And so it is contingent also to say of everyone either that they can, or cannot, imagine X. The negation of this seems not to be reducible to the form P.~P.

But then the claim that if we can imagine, or think, or picture X, then X is possible,—this looks like a claim consisting in the inference of a logical statement (concerning X's possibility) from a contingent statement (concerning what we can or cannot think, imagine, or picture). Clearly, this had better be wrong. It could be wrong in any of three ways: (1) It may be that the statement " ' X is a quadrilateral triangle ' is of the form P.~P; i.e. X is impossible " is *not* a logically true statement at all, but a mere statement of fact, i.e., the fact that " X is a quadrilateral triangle " *is* of the form P.~P. That is, we do in fact use " X is a quadrilaterial triangle " as being of the form P.~P,—but we need not have done so. (2) It may be that " No one can imagine a quadrilateral triangle " is not a contingent statement at all, but a necessary one. That is, it may be that " Someone is imagining a quadrilateral triangle " is self-contradictory, and not merely factually false. Or, (3) it may be that " If one can imagine X then X is not impossible " is not the entailment we are inclined to suppose it is.

II

Let us consider these alternatives in turn:

(1) Is the statement "'X is a quadrilateral triangle' is impossible, (i.e., of the form P.~P)",—is this statement itself a logically true statement or only a statement of fact? We really do use "X is a quadrilateral triangle" as being of the form P.~P. But need we have done so? Is not the fact that we *do* do so contingent on our linguistic habits and conventions? Does not the statement itself just describe what is factually the case?

Well, let *this* be *given* as true: *that X is a quadrilateral triangle is impossible. How* is this true? Are further observations still relevant as they are with "To swim around the world is impossible"? Certainly not. If it is true that S is impossible, —then it is logically true. There is no chance of future observations changing the verdict. If it is true that 'S' is of the form P.~P, then to deny this would be not merely false, but self-contradictory. The matter is settled by reflecting on the logical structure of negation of "'S' is of the form P.~P", —not by further observations of 'S' in action. So "S is impossible (or possible)", if true, is logically true.

(2) Concerning whether or not "No one can imagine a quadrilateral triangle" is contingently or necessarily true it might be argued: "But the contention that if X is imaginable it is therefore possible, has nothing to do with you or any other individual, or all individuals. It does not concern what particular persons can or cannot imagine. It concerns what is, or is not, imagin*able*. It is concerned with the nature of what can or cannot be thought or pictured. That some people in fact have restricted imaginations, while others have boundless imaginations—*this* is contingent, and irrelevant. What is at issue is the very structure of experience. It is of the essence of imagining, thinking, and picturing, that we cannot imagine, think, or picture what is logically impossible. What is one denying who denies this? Here is an axiom about imagination and thought if ever there was one. This does not require further analysis; it is the basis of further analysis. This insight is itself what justifies our uttering other propositions concerning what is or is not possible. And the insight is expressed in a statement which, although it recounts a basic fact of experience, is nonetheless necessarily true. Its negation may not be of the form P.~P., so it need not be a tautology; yet the statement could not be false. What would it be *like* for it to be false?"

What we have *really* just been told is that we justify that (x) (THINKABLEx ⊃ ~ IMPOSSIBLEx) by inferring it from the equivalent principle: (x) (IMPOSSIBLEx ⊃ ~ THINKABLEx). But whence came we by this prior knowledge, which, although it be conveyed in a statement formally equivalent to the first, yet differs from it in that it appears to consist now in an inference from a logical statement to a contingent one? Why then should it be conceded that *this* inference is necessarily true although not tautologous? *Must* it be granted that no deeper understanding of our acceptance of such an axiom is possible; that it is something simply seen or not seen, but not something for which one can give, or expect, arguments? *Must* we agree that this is an unquestionable condition of offering justifications and analyses for other philosophical problems? Even were this true, must we concede that nothing more can be said?

III

Is it part of what we *mean* by saying that X is logically impossible, that we cannot think it? This does not seem to be so. To say that a quadrilateral triangle is impossible is to say that its description is of the form P. ~ P. One can say *this* however, without any reference to thought; anyone's or everyone's. In fact, if X *is* logically impossible, we *cannot* form a mental picture of X, e.g., a quadrilateral triangle. But then the connection between X being impossible and X being unthinkable may only be an empirical one. There simply never has been a case of anyone thinking, imagining, picturing the logically impossible. *Prima facie* however, this is not in principle different from saying: there never has been a case of a *perpetuum mobile*. No one has ever succeeded in building one. And, given *our* physical world, no one ever will. Given *homo sapiens*, no member of that species has ever built a *perpetuum mobile*, nor thought/imagined/pictured the logically impossible. But it need not be self-contradictory to suppose either of these circumstances to obtain; it would just be false.

Perhaps we do not even *have* the concept of building a *perpetuum mobile*. But this again is a statement about what kinds of concepts we *do*, in fact, have. " I just built a *perpetuum mobile* " may be not conceivably true, but it is not logically false. And there never has been a case of anyone thinking, imagining, or picturing the logically impossible; " I just imagined something which is logically impossible " may be not conceiv-

ably true, but is it therefore logically false? "I just imagined something whose description is of the form P.~P" is not *itself* a statement of the form P.~P, although it may indeed be a false statement.

So "is unthinkable" need not be considered part of the meaning of "is logically impossible". Nonetheless it sounds far too weak to say that therefore (x) (IMPOSSIBLEx ⊃ ~ THINKABLEx) expresses only an as-yet-unfalsified contingent regularity, or perhaps just a "psychological inconceivability". For we cannot form any notion of what would count against this principle. We cannot even find a hypothetical value for "x" such that (∃x)(Ix.Tx). We cannot form any conception of what would count against (x) (Ix ⊃ ~ Tx). If we cannot form such a conception though, then (x) (Ix ⊃ ~ Tx) cannot but be true. And if it cannot but be true that (x) (Ix ⊃ ~ Tx) then it is necessarily true that (x) (Ix ⊃ ~ Tx), even though (∃x) (Ix.Tx) does not reduce to the form P.~P. "I have a mental picture of a quadrilateral triangle" is, if false, contingently false, even though we can form no idea of what it would be like for this to be true.

It is already long overdue that we should carefully distinguish two senses of the expression "is necessarily true". For these are regularly confused in discussions of inconceivability, possibility, and necessity—and they have been slipping in uncontrollably in the foregoing.

It is necessarily true that I am sitting here writing these words. And yet the statement that I am doing this is an empirical statement. How then can it be necessarily true? It is necessarily true, for me, because in fact no evidence I could now entertain could possibly shake my present belief in the claim that I am sitting here writing these words. If at this moment I had any reason whatever to doubt this, then I would have thereby been robbed of any reason to think any other empirical statement at all reliable. My ability here and now to entertain evidence for other propositions (one way or the other), depends on my present inability to entertain evidence against my being now conscious, sitting here at this table writing these words. If the latter goes, everything goes. So no evidence can count against this proposition for me. It cannot, for me, be false that I am now sitting here consciously writing these words. So it cannot but be true. If it cannot but be true however, then it is for me necessarily true that I am sitting here consciously writing these words. This was the sequence entertained just a moment ago.

But although it is now necessarily true for me that I am sitting here writing these words, it is not logically impossible that I should not be doing so.[1] That is, the negation of the statement "I am sitting here consciously writing these words" does not itself reduce to the form P.~P. So although it cannot but be false that I am not sitting here and consciously writing these words, this statement is nonetheless not self-contradictory. An exactly similar analysis can be given for such a proposition as "A *perpetuum mobile* is impossible", or "Nothing travels faster than light". That this is so shows that the first person idiom of "I am sitting here consciously writing these words" is not essential to the construction of a sentence which expresses what is both not conceivably false and yet not tautologically true. That there should be a *perpetuum mobile* invented to-morrow is not logically impossible; but there is not the slightest notion extant as to what such a device could be like. If something should be discovered to move faster than light the description of this fact would have to await the construction of a notational and conceptual framework *ab initio* before the event could be made at all intelligible to us. Such new sciences are possible. But that they are necessary before a *perpetuum mobile* or a velocity $>c$ can be countenanced makes it quite clear in what sense it is necessarily true that there can never be a *perpetuum mobile* or a velocity $>c$.

The sense then in which it is now necessarily true for me that I am sitting here consciously writing these words, or that a *perpetuum mobile* is impossible, must be strictly distinguished from the sense in which it is necessarily true that no triangle can be quadrilateral. The first two cannot be false because no conceivable evidence could show me that they are false. The last cannot be false because the very statement of its falsity is internally inconsistent. It is now inconceivable for me that I should be doing other than sitting here consciously writing these words, or that a *perpetuum mobile* should be built to-day; and it is inconceivable that there should be a triangle which is quadrilateral. But the reasons for the inconceivability differ in each case.

Assessing the logical status of the principle: *if X is thinkable, then X is possible*, then, may best be done by entertaining its negation. That negation is of the form: There is an X such that X is thinkable and X is impossible. Now we will all agree that this describes an inconceivable state of affairs. But in what sense of inconceivable? Is $(\exists x)(Tx.Ix)$ self-contradictory, or is it

[1] I.e., While I cannot negate the statement, I can entertain its negation.

simply an hypothesis for which no conceivable evidence whatever could be forthcoming? Is it of the form $(\exists x)\ (Px. \sim Px)$? Or is it simply like the statement: *It is not now the case that I am sitting here consciously writing these words*[1]? Is it necessarily true that everything thinkable/imaginable/picturable is possible, in that nothing could conceivably count as evidence against this,—or is it necessarily true in that the very idea of a counter-instance is itself self-contradictory?

IV

The former position is the more attractive because of the argument which opened section II. In 'what is thinkable is possible' we seem to be arguing a matter of fact,—albeit a very abstract one. We do in fact think in the ways in which we do in fact think; we imagine in the ways we do in fact imagine; we picture as we do in fact picture. And in fact, what is logically impossible is never discovered to be either thinkable, imaginable, or picturable. Although an alternative to our ways of thinking, imagining, picturing cannot *ex hypothesi* be known to us, it does nonetheless seem venturesome to suppose " everything imaginable is possible " necessarily true in that its denial is self-contradictory. Although we have no idea of what it would be like to think differently, it does not seem demonstrably self-contradictory to suppose that we might have thought differently. To the logically equivalent way of stating the same principle—viz., what is impossible is unthinkable/unimaginable/unpicturable—the same argument must apply. Because although it is certainly true that if X is logically impossible, then X is unthinkable, it is again questionable whether the denial of this is logically inconsistent, or simply such that we can form no conception of evidence in support of it.

The statement then that *if X is thinkable X is possible* is necessarily true although it has a consistent negation. But it is still open to the objection that it appears to be an inference from a contingent statement to a logical statement. And I cannot see that this objection is unjust. What is or is not thinkable is contingent on the ways in which in fact we think. What is or is not logically possible is laid down in the rules concerning consistency and contradiction.

What has almost certainly happened here however, is that there has been a slide in the statement of this principle from one sense of " necessarily true " to the other,—from one sense of

[1] Or like the statement: *M.I.T. built a perpetuum mobile to-day.*

" conceivable " to another. Thus we know (1) that if X is thinkable, then it is not the case that X is inconceivable. And we know also (2) that if it is not the case that X is inconceivable, then X cannot be of the form P.~P. (3) If X is not of the form P.~P, then X is possible. While each of these uses of " inconceivable " is independently legitimate, there has been an illegitimate slide from (1) to (3), from one sense of " inconceivable " to another. Although it seemed that this term was functioning as a middle term of a hypothetical syllogism, we have here really two different concepts disguised by the one word " inconceivable ". For " not inconceivable " in (1) meant that nothing could count as evidence in favour of its being inconceivable. But " not inconceivable " in (2) meant " not of the form P.~P." And so the principle needs restating. It cannot simply read: If X is thinkable, then it is not the case that X is inconceivable; and if it is not the case that X is inconceivable, then X is possible. For the former conditional and the latter one *are* of different logical types, and hence cannot function in one argument as they have been supposed to do. And yet it is exactly this logical slide which is smuggled into most statements of the principle that *if X is thinkable, then X is possible.*

Indiana University

SYMMETRY, TRANSITIVITY AND REFLEXIVITY

By S. BHATTACHARYYA

MR. DAYA in his paper 'Symmetry, Transitivity and Reflexivity' (ANALYSIS, 19.1, pp. 7–11) has tried to prove against Russell that symmetry and transitivity of a relation do not necessarily imply its reflexivity. Here we shall try to show that he has failed in his attempt, for (i) none of his criticisms of Russell's argument is justified, and (ii) the solution he recommends is no better than Russell's.

Daya begins by citing a 'concrete' example: "John is different from Tom and Tom is different from Harry", where he thinks "symmetry and transitivity do not lead to reflexivity" (p. 8). To prove the symmetry of difference in this case he says "It seems clear that if John is different from Tom, Tom must also be different from John" (p. 7); and to prove transitivity he asserts "and if John is different from Tom and Tom is different from Harry John *must* also be different from Harry" (pp.7–8. Italics ours). But we may ask here: Must John be different from Harry in this case? What if 'John' and 'Harry' be names of the same person? 'John must be different from Harry' does not follow from what he has stated, but follows from an unstated assumption that the different names, 'John' and 'Harry', must stand for different individuals. Thus it cannot be insisted that "at least in this particular instance, symmetry and transitivity do not lead to reflexivity" (p. 8), for, even in this particular instance, difference has not been shown to be transitive.

Daya of course is aware of this objection, and he tries to meet it by challenging the validity of Russell's argument to deny the transitivity of difference and other similar relations, and asserting that "the argument for denying their transitivity *has nothing to do* with the symmetrical, asymmetrical or non-symmetrical character of these relations" (p. 9. His italics). This statement, which he simply asserts but does not prove, is wrong, for it is wholly on the basis of their symmetry that these relations are asserted to be intransitive. Daya himself acknowledges this when he says "In the usual Russellian argument, it is the symmetrical character of the relation that permits the identification of x and z" (p. 10). Thus it seems that the most important argument of Daya against Russell is his argument against this identification ,which we discuss below.

Daya thinks that if the possible identity of x and z is regarded as an argument against the transitivity of difference in 'x is different from y, and y is different from z', then "symmetry itself . . . can be questioned", and "even the statement of an irreflexive relation would become logically impossible" (p. 10). But this difficulty springs from his assumption that there are only two alternatives, either (i) to allow different variables to have the same value in all cases, or (ii) not to allow this in any case. This seems so obvious to him that he does not stop to examine the definitions of symmetry, transitivity, reflexivity, etc., and rashly asserts that the possible identity of x and y in 'x is "greater or less than" y' invalidates the symmetry of the relation 'greater or less than' (p. 10). But that he is wrong can be easily seen. A symmetrical relation, R, between x and y is defined thus:

$$(x)\,(y)\,(xRy \supset yRx).$$

Now it is obvious that this definition is equivalent to:

$$(x)\,(y)\,(xRy \supset :yRx.x=y \vee x \neq y)$$

which again is equivalent to:

$$(x)\,(y)\,(xRy \supset .yRx.x=y : \vee : xRy \supset .yRx.x \neq y).$$

This shows clearly why even though the relation 'greater or less than' cannot hold between x and y when they are identical, its symmetry is not affected. There is no difficulty either in "stating an irreflexive relation". An irreflexive relation, R, between x and y can be defined thus:

$$(x)\,(y)\,(xRy \supset x \neq y).$$

Now the real nature of Daya's objection becomes clear. We have stipulated in the definition of irreflexivity that the different variables 'x' and 'y' cannot have the same value, whereas in the definition of symmetry (and, as can be easily shown, in that of transitivity) we have allowed them to have it. Apparently Daya does not want to allow this. Without stating anywhere what objection he has to our following this procedure, he asserts "Once, however, this limitation is accepted it would apply *equally* to transitivity and invalidate the argument given by Russell . . ." (p. 10. Italics ours). If we do not apply the limitation 'equally' Daya will perhaps say that we are inconsistent. But that there is no inconsistency can be easily shown by formulating explicitly the rule of substitution followed by

Russell (and by logicians generally) thus: Different variables may have the same value unless otherwise stated. Though from Daya's insistence on applying the limitation 'equally', one would expect him to accept the rule "different variables can never have the same value", what he actually says is very different. He recommends "the acceptance of the limitation that a symbol is not to be interpreted the same as another symbol, *unless allowed otherwise* . . ." (p. 11. Italics ours). The qualifying phrase 'unless allowed otherwise' is to be noted carefully, for here he is withdrawing his earlier demand that the limitation be applied 'equally'. Now if it is withdrawn, as it apparently is, then the *only* objection to Russell's argument is also withdrawn. For it is clear that Daya cannot disallow exceptions to Russell's rule of substitution while allowing them in his own case. Moreover, he is definitely mistaken when he says (without proof) that the acceptance of his rule "would hardly disturb anything except the belief that symmetry and transitivity necessarily involve reflexivity" (p. 11). His mistake will be apparent if we consider his 'concrete' example: "John is different from Tom and Tom is different from Harry". He thinks that he can now have the conclusion "John must be different from Harry", for Russell's objection that 'John' and 'Harry' may be names of the same person, cannot hold good because of the new rule of substitution. But now the difficulty is that "John must be different from Harry" becomes analytically true, and, therefore, follows from any and every sentence. For example, we shall argue correctly, according to Daya's rule, if we say: "John is identical with Tom, and Tom is identical with Harry; therefore, John is different from Harry." Here the two sentences, 'John is identical with Tom' and 'Tom is identical with Harry' will be meaningless, because they violate Daya's rule; still they will imply the (analytic) truth 'John is different from Harry'. As a matter of fact the acceptance of his rule will necessarily lead to a total change of the ordinary system of logic, for

$$(x)\,(y)\,(x \neq y)$$

becomes a theorem. Daya has not realised that the rejection of one logically valid argument is impossible without rejecting a substantial part, sometimes the whole, of the system of logic. If, on the other hand, he appeals to his qualifying phrase, 'unless allowed otherwise', and adds "'John' and 'Harry' may be names of the same man" to "John is different from Tom, and

Tom is different from Harry ", then he cannot have the conclusion " John is different from Harry ", for he has come back to Russell's position. But though by frequently appealing to his qualifying phrase, he can avoid disasters, his rule will be disadvantageous to work with. For the cases where we want the different variables to be allowed to have the same value far outnumber the cases where we do not, and Daya's rule that different variables have different values unless allowed otherwise, will mean constant addition of the clause that the variables are so allowed. Thus we find that Daya has not only failed to find any defect in the ordinary rule, but his own rule is much inferior to that which it replaces.

Now we can sum up. If exceptions are to be allowed, then Russell's rule has a technical advantage over Daya's rule. If exceptions are not to be allowed (for whatever reasons) then Daya's rule is as defective as Russell's. Thus Daya's polemic against Russell means simply that he wants to follow another rule of substitution and reject the usual one. He is, of course, free to follow his rule which, though practically disadvantageous, is still not self-contradictory, but he has not succeeded in showing why the ordinary rule has to be given up.

Saugar University

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